

CHEMICAL HERITAGE FOUNDATION

MARION DAVID FRANCIS

Transcript of an Interview
Conducted by

James G. Traynham

at

Cincinnati, Ohio

on

24 January 1997

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

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FRANCIS

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MARION DAVID FRANCIS

1923 Born in Campbell River, British Columbia, Canada, on 9 May

Education

1946 B.A., chemistry, University of British Columbia
1949 M.A., chemistry, University of British Columbia
1953 Ph.D., biochemistry, University of Iowa

Professional Experience

1946-1946 Chemist, Canadian Fishing Company
1946-1949 Instructor, University of British Columbia
1949-1951 Research Assistant, University of Iowa
1951-1952 U.S. Public Health Fellow, University of Iowa

Procter & Gamble Company

1952-1976 Research Chemist
1976-1985 Senior Scientist

1985-1990 Senior Scientist, Norwich Eaton Pharmaceuticals, Inc.
1990-1993 Research Fellow, Victor Mills Society

1993-present Consultant, Procter & Gamble Company

Honors

1977 Cincinnati Chemist of the Year Award, American Chemical Society, Cincinnati Section
1979 Professional Accomplishment Award in Industry, Technical and Scientific Societies Council of Cincinnati
1990 Technical Innovation Award, Victor Mills Society
1994 National Industrial Chemistry Award, American Chemical Society
1996 Perkin Medal, Society of Chemical Industry
1996 Morley Award and Medal, American Chemical Society, Cleveland Section

ABSTRACT

Marion David Francis begins his interview with a discussion of his childhood in Canada. Deeply influenced by his industrious parents and siblings, Francis worked his way through high school and college at a logging camp. He received his B.A. in chemistry in 1946 and his M.A. in chemistry in 1949, both from the University of British Columbia. Francis married shortly after, and he and his wife moved to Iowa, where he continued his studies at the University of Iowa, obtaining a Ph.D. in biochemistry in 1953. Francis accepted a position with Procter & Gamble in 1952. His first work there involved research on detergents and skin penetration. Procter & Gamble then moved Francis into hair research. Finally, Francis moved to the dental section, where he became involved with fluoride research. Using both human and bovine dental samples, Francis explored enamel resistance to calcium fluoride. He also proved in other lab tests on rats that fluoride had an anti-enzymatic effect on teeth, and that fluoride treatments helped protect rats' teeth from decay. Francis continued to do dental research on calculus and its safe removal from teeth without damaging the enamel. Speaking on scientific innovation, Francis touches on team effort and support, as well as management and research and development. Francis concludes the interview with a reflection on winning his scientific awards and final thoughts on his family.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

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INTERVIEWEE: Marion David Francis

INTERVIEWER: James G. Traynham

LOCATION: Cincinnati, Ohio

DATE: 24 January 1997

TRAYNHAM: Dr. Francis, according to what I've read, you were born May 9, 1923, at Campbell River, British Columbia, Canada.

FRANCIS: That's correct.

TRAYNHAM: Can you tell me something about your parents and your early childhood?

FRANCIS: Well, my father was the accountant in a logging camp. Actually, I don't remember anything about Campbell River, although I've been there. I was born, and then when I was about two, we moved to Cowichan, British Columbia, which was another logging camp. My dad was an accountant there. I do remember things like steam engines arriving for the logging, and so on. That was a very, very rural setting. We lived right beside a lake. I remember swimming there with my brothers. From there we moved directly to Vancouver, British Columbia. From then on, until I was about fifteen, I had a very city life. I went to private school for the first seven years.

TRAYNHAM: Where did you go to school?

FRANCIS: This was in Vancouver.

TRAYNHAM: Vancouver, yes.

FRANCIS: Yes. Then, my dad felt that I needed to be broadened. He was very education-oriented. He never put any pressure on me, but he talked many times to me about the importance of education. After grades one through seven in private school, I went to Kitsilano Junior High and then to Lord Byng High School in grade nine.

TRAYNHAM: How far had his own education proceeded?

FRANCIS: Oh, I don't think he even finished grade school. He was a salesman—originally, he was a salesman. That's how he met my mother. He was traveling to Canada, and he had business with her father. That's basically how the family unit started. He was about twenty-two years older than my mother. He lived to be eighty-eight.

Just right over there is a picture of some of the people who were at my mother's ninety-seventh birthday. She was still very viable. She played a mean game of bridge and could beat just about anybody at cribbage. [laughter] Her education, I think, just went through grade school—I'm not positive about that. She apparently was an honor student.

That's about all I remember of the real early years. From the time I was fourteen or fifteen, we moved again to a rural setting on Lulu Island, which is just outside of Vancouver. That was a very rural setting. I went to school there at Richmond High School up until grade twelve. I left Lord Byng High School at the middle of grade nine, moved to the rural setting on the island called Richmond. I went to the end of grades nine and ten and eleven. Then, my father was transferred out of town to Redonda Bay, to another logging camp where he was again the accountant. My mother went to live with him.

Both my brothers were by that time out on their own. I went to live with my brother at Woodfibre, British Columbia, at a pulp mill where he was on his way up in the company. He started as a laborer—ditch digger—when they were in construction. He ended up as the manager of three of their plants in British Columbia. He just kind of went up and up.

Anyway, I went to live with him. I lived in the bunkhouse for a year and went to an extremely rural school. I think there were only two grades in my school. We had a one-room schoolhouse. There were four in grade twelve—I was one of the four—and there were three in grade eleven, in that one room. I had two marvelous teachers, unbelievably good. Mr. Ralph Ferguson, who was my math and science teacher, was very good. He was also the basketball coach. I played basketball for the team, and we won the tournament by one point, which I got on a foul. [laughter] Anyway, that was an extremely rural setting. It was very nice. I lived in the bunkhouse with my brother. At that time, I don't think he was married.

From there, well, I completed grade twelve. Then, I went to the logging camp to live with my mother and father, and worked. I began as a bull cook. I don't know whether you know what that is. That's basically making beds, sweeping the floors, lighting the fire in the cook house at 4:45 in the morning; and getting all the wood, the kindling for the cook, and the wood for the bunkhouses, and so on. [laughter] I lived with my mother and father there. That was a wonderful experience. I've no regrets about that at all. It was marvelous.

TRAYNHAM: How many years did you do that?

FRANCIS: Just one. Well, I did it more than that. I came back every year from college, except for one year. My brother Pat was in business for himself and very, very successful—he did not think, “high school.” He just felt there was no objective. He was a businessman from the word go; he had an extremely successful business. He could have retired in his forties if he’d wanted to. He was in the Air Force, as was my other brother, during the Second World War. He came back with five thousand dollars in gratuities from the Canadian Government from serving in the Air Force. With that, he bought a one-third interest in a ready-to-wear clothing business. Then he bought his partner out in three years. Then he had his own business from then on (Francis Agencies).

I worked with him one summer as a salesman. I was successful, but boy, did that teach me that that was not for me. It was a wonderful experience because I did fairly well, but I hated it—every minute of it. Selling to people I felt really didn’t need to be sold to, but it was part of the game, so to speak. I think that really solidified my tremendous desire to just go on as far as I could go.

TRAYNHAM: Neither of your brothers went to college, then?

FRANCIS: Oh, my eldest brother, Bill, who is really my mentor—has always been my mentor—he got to the first year of university. He had the top scholarship in British Columbia. He won that and was going to go to a special school, which sponsored the scholarship, but my parents’ financial condition was so bad that he had to leave and go to work. That’s when he went up to Woodfibre, British Columbia, Canada.

TRAYNHAM: How much older was he than you?

FRANCIS: He’s eight years older than I am. My brother Pat is, I think, four years older than I am. Bill is—I think he will be eighty. I’m pretty sure he’ll be eighty in April. My brother Pat will be seventy-seven, I guess, or seventy-six. I don’t keep close track of ages. [laughter] Of course, I’m seventy-three.

TRAYNHAM: Then, after your year living with your parents at the logging camp, you did what?

FRANCIS: Well then, I went to the university each year. I had no money. My parents couldn’t support me. So I worked for a year in the logging camp, got just barely enough money to see

me through the next year. Then, that whole year I went from bull cook to whistle punk. Bull cook is the lowest on the total totem pole in a logging camp; whistle punk is the next lowest. He's the one who gives all the signals to the rigging slinger and the choker setters and the donkey engine puncher who has the power equipment. I just went up and ended up as a loader. My salary kept going up from the lowest. I think I got ninety dollars a month or so, I can't remember. It was a very low amount when I first started. It did give me enough to get to college and go through. Of course, fees at that time were much lower than they are now. [laughter] I'd never have made it on today's whatchamacallit.

At any rate, at the end of that year, I went back to college. I lived in town with very close friends of my mother's and father's, Mr. and Mrs. Thomas Byrnes. They were very wonderful to me. There was one other young man, Frank Haney, who was taking engineering. I was taking an honors chemistry course.

TRAYNHAM: What prompted you to select chemistry as your major?

FRANCIS: Oh, I guess there were a number of things. In grade eight, I went from the private school, which had no science at all, to the big school. It was a huge school, Kitsilano High School. There was a teacher whom I took a science course from—and some of my teachers there were fabulous. I mean, they were really inspirational. The history teacher was great; the social studies teacher, who was a little separate from that, was great.

The math teacher really inspired me. He was tough, but he was great. I was way behind. When I went down there, I didn't know what an x or a y was, in terms of algebra. They were already well into it. He was just appalled by the fact that I had not had even the primary things. I didn't even know what to do with an x or a y, and didn't know what it meant. He really helped me out. I passed without having to write the examination. We had all of our records and so on, but I managed to get—I think we called it, "recommended," in those days. I didn't really write any final exams. Then I went from there, as I mentioned previously, to Lord Byng High School.

At any rate, all through my history there have been teachers who—I haven't been close friends with them—but they have been incredibly inspirational, really inspirational. When I was with my brother in the one-room school, the science and math teacher there was extremely good—Mr. Ferguson. Mr. Lutak was the English teacher, and despaired for me in the poetry area. I loved poetry, but he insisted on—he would read a poem twice, or he would put it on the board for a set period of time, and we had to repeat it with all the punctuation perfect. I had problems with that. He used to say, "Francis, I just don't understand why you can't handle this." [laughter] He was a good teacher and I enjoyed the courses, but he was a hard taskmaster.

Well anyway, I went four years. Each time, I would say, I had an objective. I really didn't think I was bright enough to get through my first four years of college, but I had that as an objective. Each year when I got through, I'd say, "Gee, you made it."

I finally got my bachelor's. My dad came down for that. It was wonderful. He came down to town especially for that. Then, my next objective was a master's. That was postwar years. Canadians really had trouble getting into U.S. universities. The U.S. universities were thought to be way up here, and Canadian universities below, not as good academically—it wasn't true, but that was the impression.

TRAYNHAM: Before you go on with your account of your master's degree work, let me back up a moment. You had referred to the excellent teaching you had in math and history. Where did you find the interest in chemistry?

FRANCIS: Well, as I said, in grade eight when I made the transfer from the private school, there was a science teacher. He actually ran experiments in the laboratory. I kind of got turned on doing some of his experiments. That was the first that I really remember. Then my professor of inorganic chemistry—Professor J. Allen Harris—I attribute the fact that I got my bachelor's, my master's, and my Ph.D., all of those things, to him. He failed me in one of his courses. It was his advanced inorganic analysis of the rare elements. You know, the reason I failed was, he was a hard taskmaster. The quantitative part of it was very exacting—long calculations. When I wrote the final exam I had no problem with the qualitative, because I loved it. You know, it was molybdenum, vanadium, uranium, and all of the rare elements. It was really fun separating them and identifying them. We had unknowns. But the quantitative part was very long and involved. When I wrote the exam, I knew how to do these things. But because the time for the exam was so tight and these were so long, I set the whole solution equation up, but then didn't take time to do all the calculations. We didn't have calculators in those days; it was finger work. [laughter] I set them all up and set the final solution equation. Then, I went on to the next problem to show that I knew how to do the problems. Well, then when I got all finished, I went back and started to get my final answers. Well, I think I got three out of six or four out of the six. I didn't finish the actual final calculations.

I went to him and I said, "Well, you can see that I knew how to do it. I don't understand why you failed me, because I knew that from there on, it's just mechanical. That doesn't require brains; that requires stick-to-itiveness." He said, "I'll tell you why. When you go out of here, if you go to work for a company and you hand in a report with just calculations but none of them finished, they'll fire you." He said, "That's why I failed you."

I went up to the logging camp. Each summer, I went up to the logging camp to make enough money for the next year. I wrote him and said, "You know, I guess I should just quit and go to work." He wrote me back an absolutely lovely letter. I don't know why he did, but he did. He wrote me back a letter and said, "Don't let that faze you. Come back." We were able to go back and write supplementals. That's the only course I ever failed in my life, but I sure failed it. [laughter] So he kind of inspired me that maybe I had what it took to finish. I went back and I wrote the supplemental, and I passed it. He was, of course, my professor. Then

he became the professor for my bachelor's thesis. I wrote a bachelor's thesis, a master's thesis, and a Ph.D. thesis.

TRAYNHAM: What was your bachelor's thesis subject?

FRANCIS: It was on the inorganic—well, it was actually the analysis of zinc ores. In the process, I ran across a really unique reaction with the ferrocyanides, because I was using that to take out some materials. I ran across a unique one and did the kinetics of it, and handed it in not as part of my thesis, but as a supplement to the thesis. You know, with a reference, “This is an interesting thing.” He gave me a very high mark—not for my thesis, which I guess you'd call very routine and not terribly brilliant. But the little supplement, he said, “You and I have got to publish that. That's definitely publishable.” Then, I did my master's thesis under him also. That was a continuation of that little supplemental work and so on. From there, of course, I came to Iowa to get my Ph.D.

TRAYNHAM: What led you to choose Iowa?

FRANCIS: Well, very practical considerations. I had applied to Iowa, to Purdue, and to the University of Illinois. They all accepted me, but Iowa—see, I was in love. Well, I met my wife at the tennis club and fell in love. The last time I was up at the logging camp, I came back down and we were married right after that. Then we left for Iowa.

TRAYNHAM: You can mention the score love in tennis.

FRANCIS: Well, I scored well, let's put it that way. [laughter] She was a wonderful woman. She stayed by me all the time, and it was hectic. We had so little money—so little money.

TRAYNHAM: Was Iowa's offer better than the others?

FRANCIS: Yes. They offered housing, and not a scholarship, but an assistantship for chemical research. I think it was called a research assistantship. Also, another consideration was, there were good hospitals there. My wife was a tech. She was a medical tech and had been a medical tech in Vancouver, and had very good references. She got a job immediately.

We arrived in Iowa City. I guess you weren't at the Perkin, but we arrived in Iowa City with seven dollars and sixty-seven cents total to our names and a broken-down car, because we

drove across in a little Morris tourer. It broke down in Salt Lake City. We were conned, unfortunately, by the guy who took care of our car, so it broke down again about fifty miles out of Salt Lake City after we had gotten going again. We limped into Iowa City at twenty-five miles an hour; that was the fastest we could go. Of course, we caused all kinds of havoc on the highway with the big trucks, because when we geared down to go up a hill, we were going less than twenty-five miles an hour. It was kind of a hectic trip.

Anyway, each time, I just really didn't think I was smart enough to even think of getting a master's or a Ph.D. But when I got my bachelor's, I thought, "Well, maybe I can make it." Then, my target was a master's. That took three years, because I was an assistant and research advisor at the University of British Columbia. That's what put me through the last three years of my master's. I worked pretty much full time, other than when I was taking my courses.

I got my master's. Then when I got my master's, my professor—I was going to stay and take my Ph.D. there. He said, "Francis, forget it. I will absolutely under no circumstances recommend you for a Ph.D. at the University of British Columbia. You know every single professor here. You've taken myriad physics courses, chemistry courses, and math courses. You know every professor. You're not going to learn anything more. You get out of here. I'll recommend you for any university in the United States, but not for here. I will just blackball you if you try to stay here." [laughter]

TRAYNHAM: That was very firm advice.

FRANCIS: He shook me up, quite frankly. That's when I applied.

I had another diversion that was kind of interesting. My life has been so lucky. I mean, I can't really believe how lucky I've been with people I've met. When I got my master's, my parents needed money still. They were not very rich. Dad worked hard. Dad was in his seventies, maybe. I guess he was deep in his seventies at that time. He wasn't a young man. He was working ten, twelve hours a day a lot of the time. I was watching ads, and I applied for a job in Bolivia as an analytical chemist. They wouldn't accept me because I didn't have Spanish, but they said they would put me on the reserve list. Well, I had my applications in to the universities.

They came back, and I was accepted at the three. I waited as long as I could to accept this Bolivian job, because the pay in my mind at that time was incredible to go down there. At any rate, I waited as long as I felt I could, and then I accepted at Iowa. Two weeks after I accepted at Iowa, I got an offer from the big mining company in Bolivia. Of course, I wrote to them and said, "Thank you very much, but I have already accepted at the university."

That's how I accidentally sort of ended up at the University of Iowa, among other things. There again, I was inspired by the organic professor and the physical chemistry professor—

Professor George Glockler. He was marvelous. Dr. Stanley Wawzonek was the organic chemistry teacher. He was feared. I mean, he was. I can remember on one exam, he marked mine wrong—one of the really major questions. It knocked me from way into the nineties down into the seventies. I went back over what I had done. I was convinced I was right, even though it wasn't what he had wanted, apparently.

I talked to my buddies. They said, "Don't go to Wawzonek. If you go to Wawzonek, he'll fail you offhand if you complain." I thought about it for a while, and I thought, "The hell with it. [laughter] I'm right. I know I'm right." I went back and I explained to him, my part of this organic synthesis question he'd given. He said, "You're right." I got my nice A or ninety, or whatever it was.

TRAYNHAM: Did that change the mythology about Wawzonek among the graduate students?

FRANCIS: It did for me, and I certainly told my companions. I said, "If you're right, you're right, and he will agree to it."

Then, of course, the really big event in my life—and I mean to say, it was big—was my research professor, Dr. Harold Boaz

. He was a young guy, a very, very good guy, really hotshot. I was about a third of the way into my thesis for my Ph.D. The problem was the analysis of proteins using fluorescence methods, which weren't developed at that time; they are now. He was a very good physical chemist and he was the professor of analytical chemistry, so I was doing my Ph.D. research for him. I was about a third of the way through. I'd submitted my first—I guess you'd call it the first year's terminal research report to him. He'd given me a good mark on that.

He walked into my laboratory on a Saturday morning, when I was there. It was about ten o'clock in the morning. I was working, doing my research. He said, "I'm sorry, Dave, but I'm leaving. You'll have to find another professor." Bang, I didn't have a research professor. I was partly into my Ph.D. work. See, my major was analytical, and I had two minors, organic and physical. It was two weeks before the comprehensives. [laughter]

I immediately looked around. There was nobody else whom I really wanted to work for. There was a physical chemist; there was an inorganic chemist. I'd taken courses from them. I wasn't really inspired by them, didn't really care for them. I didn't think they could give me the kind of advice I needed or at least wanted, let's put it that way. They probably had it, but I didn't see it, shall we say. So I immediately sent out feelers. I was all set to go to the University of Washington out in—what is it? I forget where it is; it's just a little small town. There's one in Seattle. There's another one. The major one is in Seattle. To transfer there meant I really would almost start my Ph.D. all over again. I would lose almost a year and a half.

I got a telephone call from a guy in the radiation research lab, a professor biochemistry

whom I'd taken a one-unit course from in the use of isotopes in biological chemistry. I loved it. Just for interest, I had taken some biochemistry courses from a couple of other professors, just as a fun thing to do. In the last year at the University of British Columbia, I took a major biochemical course from Professor Eagles. He was the most dynamic teacher I've ever run into in my life, even though biochemistry wasn't my desire. I had no idea I would go into biochemistry.

Anyway, this professor at the University of Iowa called me up and said he'd heard I was leaving. Would I come over and consider majoring in biochemistry, and doing a research project under him? I went over and talked to him. I said, "I don't even have a minor in biochemistry, and it's two weeks before the comprehensives." He said, "I think we can probably arrange it."

I did. I wrote my biochemistry. I don't think I passed it. I don't really think I passed it, but I guess they made an exception. I had very good marks in other things. I don't think I passed it, but nobody ever told me I didn't. [laughter] I had major blanks in my biochemical background, except for this one course from Professor Eagles in British Columbia. It was tremendous.

I transferred to the radiation research lab with Dr. Theodore Winnick. He was my research advisor. I had a wonderful, absolutely wonderful Ph.D. thesis. I did some things that didn't really come to light until twenty to thirty years later, that I didn't have time to pursue, so we never did publish it. But he and I published my thesis in *Journal of Biological Chemistry*, which at that time was considered the apex of biochemical journals (1).

[END OF TAPE, SIDE 1]

FRANCIS: What we were doing was determining whether or not proteins could be absorbed in the gut whole, or what happened to them. We used radioactive materials and isotopic dilution so that if they had to be broken down to the free amino acid, then they would isotopically dilute the tagged amino acids. Then, we would be able to determine. We used chromatography to separate out the separate amino acids, and then determined the radioactive level of that amino acid. If the proteins were broken down, then they would produce isotopic dilution of the tagged amino acids. That was my thesis. That was a fun time. It was mostly tissue culture work. We used chick embryos and chick embryo hearts. We did a little bit of work on lungs, but mostly it was on chick hearts.

I got my thesis. Then, I sent out feelers. A lot of the guys, really—I guess, almost were having fun trying to find their jobs—they would do seven, eight interviews. I just made up my mind that that was a waste of my time. I wanted to go to one of three different places—I mean, final.

Again, the reason I went to P&G [Procter & Gamble] was purely happenstance. I mean, you talk about lucking out. [laughter] I was in the radiation research lab, as I mentioned. Joe [Joseph] Callen—Dr. Callen—was interviewing at the biochemistry department, which was separate, across the river in Iowa City. See, I was in radiation research across the river from the biochemistry department. He was interviewing for students for P&G who would be graduating with the Ph.D. He called up the head of the radiation research lab and said, “I’d really like to come and see the laboratory facilities and see what you’re doing.” The head of the lab said he was busy and came to me—I was really deep into my research at that time—and said, “Will you take time out and show him around?” I knew everything that was going on in the laboratory. I knew the iodine-131 work and all this stuff that was going on, because I knew everybody in the lab. I admit I was a little resentful, because it meant a chunk of about three hours right out of my research time. I was spending twelve, thirteen, fourteen hours in the lab at that time. I said, “Sure, I’ll do it.” I made up my mind I’d do as good a job as I could, but I had no interest in P&G—none whatsoever. I’d written DuPont and Eli Lilly, and I don’t think I wrote to anything else.

Anyway, I gave him a tour of the laboratory and explained what was going on with the I-131 material for the thyroid, using it as a knockout for hyperthyroidism. I explained to him my research and the research of most of the other Ph.D.s who were doing their research there.

He went back. Next thing I knew, I had a phone call from the research director [Dr. N. Beverly Tucker] of Procter & Gamble with a letter, inviting me to come in to interview. I thought, “Well, I don’t know. I know P&G is a big company. It’s a good company, but I can’t imagine working for a soap company”—because that’s what I thought of P&G, as a soap company. In fact, my brother still heads his letters to me, “Dear Soap Maker.” [laughter] He loves to do it because he knows it irritates me.

Anyway, I went in and interviewed. Then they invited me in for a second interview. I told them, I had only interviewed at Eli Lilly. They made me an offer. I didn’t get around, ever, to going to DuPont. I wanted to go, but I didn’t get around to it. I finally accepted at P&G before I even went to DuPont.

My research advisor desperately wanted me to go to Lilly because he was a consultant for Eli Lilly, and he wanted me in there as someone he knew he could rely on. It must have been their personnel department. I guess I was a feisty son of a gun. Anyway, I interviewed. They made me a very nice offer. I said, “Well, I haven’t interviewed at DuPont, and now I’ve got an interview with P&G. Can you delay?” They said, “Yes, but we’re very desirous of having you.” That letter was okay.

The next letter that came was no more than two or three weeks after I’d told them that I hadn’t interviewed at DuPont and I couldn’t go for another month. My research wouldn’t let me go. They said, “Look, we have two other very brilliant people whom we’re holding off offering, waiting for you to tell us. Would you please, if possible, give us your answer?” I wrote back and said, “Gee, if these guys are so good, why don’t you just go ahead and hire them? [laughter]

I have to wait until I've interviewed at DuPont and P&G," because I'd told P&G I'd come. I guess I cut my motors as far as Eli Lilly. I never heard a word from them after that. Anyway, that's basically the story. Then of course I went to P&G, and I was in skin research, my first job at P&G.

TRAYNHAM: I presume that at your interview with P&G, you saw it was an enticing place to go.

FRANCIS: Well, their research facilities looked awfully nice. They took me out to their new Miami Valley Laboratories. That was in 1951. See, 1952 was when I joined them. I interviewed in 1951, I think it was. They showed me the laboratories and showed me all the plans, and what was involved with the animal laboratory, and all of the things that would be available.

TRAYNHAM: When you went to work with P&G in 1952, your Ph.D. was formally awarded in 1953. I believe you had completed all the requirements, including your dissertation. It was just a matter of when the commencement was held.

FRANCIS: Yes. I mean, I had the defense of my thesis; I had submitted my thesis, complete. My research advisor and I had already submitted our manuscript to the *Journal of Biological Chemistry*. It wasn't accepted at that time when I left, but everything was complete.

TRAYNHAM: All right. You went to P&G, then. I presume you went without any regrets at having turned down the pharmaceutical companies.

FRANCIS: No. I never did go down to P&G's old facilities. By that time, the research building at P&G was brand new. They had had an official inauguration of everything. I just jumped right into a brand-new lab. They gave me a technician immediately to work with who was very good. He was quite accomplished. He was an older person, had been around and knew where everything was and how to get things, and how to order things. He was a big help to me.

I had a very successful three years in skin research, mostly using radio isotopes and tracking the penetration rate. I set up a special system where I could actually measure quantitatively the rate at which detergents passed through skin. In that process, I discovered a rather amazing one, unique one, which then actually ended up in some of their products. What it did was, it apparently coated the skin and blocked the penetration, so that the penetration really was minimal.

I did protein determination and carbohydrate determination on the—I don't know whether you'd call it the solution that passed through. See, there was an isotonic solution below on the dermal side, and a detergent solution above on the epidermal side of the skin surface. I determined that the release of protein and the release of carbohydrate from the skin, which was of course then dissolved, were more or less not mathematically proportional, but closely proportional to the amount of penetration. The more penetration, the more the skin was damaged—or if you like, dissolved would be a better way to say that.

I noticed that as I found this one—it was called AE₃S, which was alkyl triethyl ether, a CH₂CH₂O type of thing, sulfonate—C₁₈H₃₇(OCH₂CH₃)₃ O•SO₃Na—that that little ethylene ether on the end of it really contributed to mildness, and that it cut down penetration tremendously. Since I was using radioactive materials, I could measure the detergent that penetrated quantitatively. I noticed also then, protein that dissolved and carbohydrate that dissolved went way down, which meant the skin was not being damaged as much. Then another person, Dr. Donald Opdike, translated this to actual, live guinea pigs. He showed this, then looked at the skin, and did histology on the skin with these things and found that indeed the skin was really preserved, even though the skin was offended by long treatment with the detergent.

TRAYNHAM: Before you went to the live guinea pig experiments, what was your source of skin?

FRANCIS: Guinea pig skin. I set up the first percutaneous flask so that you could actually measure quantitatively. It was just very simple. I had the shop make a ground-glass tube. I'm not an organic chemist, but the three-pronged ones that have ground-glass stoppers—well, I had the glass blowers make a ground-glass tube. Then they ground the glass very carefully on the end of it. Then the shop made me a steel ring that set right up against the ground-glass end. Then I had little prongs so that I could take elastics [rubber bands], which is what I use to hold the steel ring and skin, and could put the skin with the exterior surface in contact with the soap or the detergent, whichever it was. Then, I had Ringer's solution on the other side for the isotonic solution, so that there was not an offensive solution on the dermal side. So I could measure the actual transposition of detergent through the skin.

TRAYNHAM: This research on skin penetration by detergents was your first research for P&G, then.

FRANCIS: Yes, three years in that. I found some very nice mildness agents and a number of other things like that. Then they transferred me to hair. [laughter] If you look at my solar panel, you know that it had nothing to do with regeneration of hair.

TRAYNHAM: We trust that this was not responsible for the lack. [laughter]

FRANCIS: Anyway, I did some really fun research in hair—primarily, again, doing rate studies, where I was looking at the oxidation and reduction of the keratin of hair, which controls curling. See, that was P&G's interest, curling, and also of course straightening—what caused straightening. I did a lot of work on that.

That was a sad period for me. I mean ultimately, I still regret it terribly because I did, I think, some really outstanding work. I had a section head. Again, it's because I'm a feisty son of a gun, and I regret it now. But I handed in a report. I handed in a second report. That came about six months later. They were good research reports, and I wanted to publish them because it was very publishable material. There's no question in my mind that it was publishable—still is, probably, because it's never been done as far as I know. Then, my technician and I finished another section, and I handed that in. This was now a year and a half since the first one was handed in. He'd never given me back my reports. We had to hand them in to the section heads. They'd go over them, send them back with corrections or whatever, critiquing them.

Anyway, when I handed in the third one, I finally went to him. I said, "Look, you've got three reports. Now, one of them is a year and a half old; one of them is about a year old, or three quarters of a year old; and this one. I haven't heard word one from you." He said he'd gone over them. He said, "You're going to have to redo them and compile them all into one report." I said, "The hell with it." [laughter] See, he had my reports, so it wasn't unethical from the standpoint of P&G's interest, because they had that, but I was so fed up with the fact that I couldn't get him to give me back my reports. When he said I had to redo all of it, I just said, "Phooey on it."

TRAYNHAM: After you had decided that the request from your supervisor for rewriting your reports was unreasonable, you say you threw it away.

FRANCIS: Well, I guess it wasn't unreasonable. I mean, maybe I was unreasonable.

TRAYNHAM: What happened then?

FRANCIS: Well, they transferred me to the dental section, which was just in its very initial formative stage. There was no dental section before that. They transferred me into the dental research area. That's how then I became involved in the fluoride research. That's where I made the first really significant discoveries of the effective and extremely thin film on a bulk solid of, in this case, hydroxylapatite.

TRAYNHAM: Now, was your transfer initiated by a request from you?

FRANCIS: No, no. It was a company need. See, they wanted to get a dental section started, so they transferred me into that. They transferred: a physical chemist; I was a biochemist; a dentist; and an inorganic chemist, Bob [Robert] Grabenstetter, a really nice guy. They transferred those four core people into a dental group with a section head. The section head was Bill [William J.] Griebstein, who was just a tremendous section head. I mean, I did so much, like thermodynamics and real physical chemistry, when I was in the dental group. When I'd submit my reports to him, if I ever got them through him, I just relaxed. [laughter] He was not innovative, but he sure was a wonderful critic—a technical critic. I don't know how many times he would say, "Dave, you might know what's going on here, but you're going to have to elaborate this in order to make anybody understand it." I'd go back, and I'd rewrite and enlarge a little bit. When it got through him, I knew I was safe, because he was a heck of a good physical chemist.

Anyway, I transferred to the dental group. There all I can say is, looking back on it, the milieu was wonderful—marvelous. Dr. John [A.] Gray, who was the basic physical chemist—Dr. John Gray—he wasn't really a thermodynamicist, but he was a really good rate person. His bag was rate reactions. Al [Alfred H.] Meckel was the dentist, and he was very innovative. Bob Grabenstetter was a very bright guy who had just an immense background in the dental area. Whenever you needed to know, "Was something done?" you wouldn't go to the literature. You'd go to Bob, and he'd say, "Oh, look it up in such-and-such and so-and-so." He'd maybe have some papers on it, and he'd give you that.

Then, of course, there was a tremendously well-known—oh goodness, don't tell me my memory's going to fail me. It is. [laughter] It'll come back. He was the man who developed the phase diagrams for the calcium phosphates—Dr. Oscar [T.] Quimby. He had publications and was extremely knowledgeable. I spent quite a bit of time with him, where he brought me up to speed. I knew nothing about this—I mean, I didn't even know what a tooth was made of when I transferred in.

What with John Gray, and Al Meckel being the dentist, and Dr. Quimby—Dr. Quimby wasn't in the dental group, but he was the physical chemist who had been working on these. See, phosphates were the builders for the detergents. In other words, they were the things that scavenged the calcium and magnesium out of the water. That's why Oscar was working on these very, very theoretical phases that calcium phosphates could go into. Of course, the polyphosphates were the ones that we used as builders at that time. He filled my background in on phase diagrams, which I knew nothing about at the time, and taught me the different phases of calcium phosphate. That was a tremendous backlog for me to learn, too.

All I'm saying is that what I walked into wasn't just a terrific group. We set out to be

the best dental researchers in the world, and we ended up being the best dental researchers in the world. Bill Griebstein and Al Meckel published the first fully revealing structure of dental enamel (2). That's still a landmark in that area. John Gray published the rate reactions of calcium phosphate with various buffers (3, 4). That still is a landmark. Then I did all of the surface phenomena and the thermodynamics (5). All of those are published. Most of my publications are in the dental and medical fields.

The real breakthrough came—I don't even remember how it happened. I think someone made a trip (Dr. Willy Lange and Dr. Griebstein, and I made several trips to Zurich, to the University of Zurich. Dr. [Hans] Mühleemann was the head of the dental department at that time. He had found this material, ethoduomine, which we finally were able to get the structure of. We found that it protected acid dissolution of enamel.

Well, that put me on the road. The way I've done all my research was to try to understand what goes on in any set reaction that seems to be advantageous. I set out to find out, why was this protective of enamel in an acid solution. That led me to another compound—we did some syntheses—a quaternary fluoride compound, a methyltriethanol ammonium fluoride, which is a quaternary. That also tremendously protected the enamel, even more than the ethoduomine. It's really startling. We couldn't figure out why. I did electron microscopy, probe studies—a whole bunch of other things to see what the surface was (6).

I found that what was really happening was, we were forming a non-crystalline layer about somewhere—we figured, we didn't know for sure—between ten to fifty angstroms thick of calcium fluoride, noncrystalline calcium fluoride: in other words, amorphous calcium fluoride. Let's say "amorphous" because grazing angle, electron diffraction, and x-ray—none of these things showed it up. You couldn't see it. All you could see was the underlying apatite. But I was able to prove that that was exactly what was going on. Of course, then I started looking in the literature. I found out why aluminum airplanes don't disintegrate. They should disintegrate, but they don't.

That, again, is luck. I ran across some obscure papers (7, 8) that showed the relationship between the resistivity of pure aluminum and then aluminum where they were able to control the thickness of the aluminum oxide (Al_2O_3) film. The resistivity of the aluminum went up about 10^{16} with a fifty-angstrom layer of aluminum oxide on aluminum.

Well, the sun came out, and it verified. Then I went and I found, "Well, that's what happens with iron. When they store iron outside, they put oil on top of it. Well, that blocks the diffusion of oxygen into the iron and blocks oxidation (rust)." I started then thinking about diffusion. "Why does the enamel not dissolve? Well, the fluoride layer blocks diffusion of the phosphate and the calcium out. It doesn't block hydrogen ions into the enamel—hydrogen ions are too small—but it blocks the diffusion of the calcium and the phosphate out." Then, I also found that because the compounds were blocking crystal growth of the calcium fluoride, it was able to make such an intimate diffusion barrier. The particles were so small that they were

packing tightly in a very thin surface film, causing a diffusion barrier (6).

What I did then was, I did a series of rate studies where I could change the particle size (6). I could see it by electron microscopy. The bigger the particle size got, the more there would be destruction. The smaller they got, the less destruction there was. Again, that pointed to a diffusion barrier as being an intimate, tiny, tiny thing. We figured the particles couldn't be more than ten, fifteen angstroms in size. We did grazing angle electron diffraction. We did everything to try and actually prove that it was calcium fluoride, but unsuccessfully.

That's why I turned to thermodynamics. I said, "None of the physical technology nowadays is able to detect this thin layer, if it is there." Nobody's ever done it, as far as I know. I turned to thermodynamics, because if the surface layer was really forming an intimate barrier on the surface almost completely, then as that formation became more and more effective, the solution in contact with that would only "see" that surface and wouldn't see the apatite underneath. I did thermodynamic studies with just buffer acid and then with buffer acid and fluoride compounds. Sure enough, every time I did these studies, the dissolution would go up. Then it would just stop cold.

Then I went way out. I did, I don't know, some three-month studies. I did the actual thermodynamics to show solubility—that it reached the solubility product of the surface layer (7)—but we still couldn't see it or identify the surface layer by physical methods. We reached the solubility of calcium fluoride. Then I did a bunch of theoretical calculations, mostly to convince myself what was going on in terms of the amount of surface that was converting from apatite to calcium fluoride. When I did those, I'd have to show you the graphs, but the solution chemistry kept telling me that I should go in a certain direction because those were the largest changes in the solution characteristics.

But when I did the theoretical calculations, it was exactly the opposite. Where I was getting the largest calcium and phosphate ratio changes was the thinnest, least layer of this new "phase-on-phase," which is what I call a phase-on-phase. But if I went to this other direction, I would show it (6). That's when I was able to show thermodynamically that the surface came to the solubility product of slightly soluble calcium fluoride, not the solubility product of apatite. With buffer acid alone, the solution came to equilibrium with brushite (calcium monohydrogen phosphate dihydrate), which is only very slightly soluble in acid. Thus in each system, buffer acid alone or buffer acid plus fluoride compound, the newly formed insoluble thin surface phase protected the underlying soluble apatite phase from the acid.

TRAYNHAM: Were these studies done on teeth as a source of apatite, or were you using apatite from another source?

FRANCIS: No, no. I used both human and bovine. My major source was bovine. See, I would polish them very, very highly so that if you did grazing angle electron diffraction, you could see

nothing, nothing whatsoever. Grazing angle electron diffraction wouldn't even show an apatite pattern. If you had projections of apatites sticking up on the surface, grazing angle electron diffraction would pass through and give you an apatite pattern, because that's a very characteristic pattern by electron diffraction as well as by x-ray, obviously. We polished that to the point where we proved that there was no pattern. That was really smooth. Then we used those.

What I did was to cut them to a certain area—an exact area—so I knew the surface area. Then I could determine what was the dissolution per unit area of the apatite. I used both bovine teeth and human teeth. I sort of did most of it with bovine and then moved over, just because people would ask, “Well, what happens when you use a human tooth?”

TRAYNHAM: Was this the research that led to the incorporation of fluoride in toothpaste?

FRANCIS: Yes. See, I was able to show that what was really going on was the surface layer of calcium fluoride. If you just use sodium fluoride, you've got a huge cubic crystal that's sitting on the surface, which gave no protection to the surface at all. Dr. Bill [William W.] Briner and I did enzyme studies too, to show that enzymatically, the fluoride had an effect—an anti-enzyme effect (9). That wasn't the major effect on it, though. During this time also I was doing in vivo studies in rats to show that the fluoride treatment was really protecting the teeth of rats on a cariogenic diet (10).

TRAYNHAM: During this period of your research, were you associated in any way with Professor Joseph Muhler at Indiana?

FRANCIS: Oh, yes, Muhler. Yes, we were associated. I didn't think much of Dr. Muhler. [laughter]

TRAYNHAM: I recently saw an obituary in the *New York Times* (11).

FRANCIS: Oh, has he died?

TRAYNHAM: Yes, just this month. At least, the obituary appeared this month. The obituary credited him with the introduction of fluoride into Crest toothpaste.

FRANCIS: He was very, very dominant in that area, yes. Very dominant. P&G had long

contracts with both him and Dr. [William] Nebergall. Nebergall was the one who perfected the abrasive calcium pyrophosphate. I don't quite know how he did it, but there was a sintering process and so on that Nebergall developed, so that the particles of calcium pyrophosphate were less damaging as an abrasive. You need an abrasive, but you don't want to damage the teeth.

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FRANCIS: The older abrasives were mostly calcium carbonate, things of that nature. They were quite damaging to the teeth. My mother had grooves in her teeth, real Vs, big Vs in all her teeth from damage by it. She had her teeth right up until the time she died, but she sure did have grooves in them.

Anyway, Dr. Muhler was definitely a very, very major source of P&G's work in the clinical fluoride area. He did most of the clinicals for us. He had a big group at Indiana University. I didn't care for him, because he wasn't really research-oriented. He was very clinically-oriented. That was proper for him, but it wasn't my bag, shall we say. [laughter]

TRAYNHAM: Where did this research on fluorides lead to, then?

FRANCIS: That led to the crystal growth inhibition phenomenon. It was with the fluoride compounds that I first discovered had the effect on crystal growth inhibition. Then when Dr. [Willy] Lange of our—I forget what department he was in. He was kind of a research senior. He would go around Europe visiting the various organizations, and he visited the Henkel Corporation. Henkel gave him EHDP, which is the ethane-1-hydroxy-1, 1-diphosphonate. He brought that back. At that time, I'd now finished a lot of the fluoride work and the enamel work and had moved over into calculus. What was causing calculus? What was it composed of? In connection with Al Meckel, what was the damage? Why was it a bad thing? I found that it was literally covering the supra-gingival surface. Then, infection would set in underneath the calculus to produce periodontal problems.

I was looking at all kinds of chelating agents that would remove the calcium of the calculus. But I found that every one of them that I'd used damaged the teeth—because then I'd go to these highly-polished enamel chips and prove that it would damage the enamel, too. If it removed the calculus, it also removed the tooth. So Dr. Lange came back—he and Dr. [Homer] McCune, came back with this ethane hydroxy diphosphonate as a strong chelator. They had the intention of using it as a builder for detergents. Indeed it was a very effective builder, which would chelate calcium and magnesium and allow the synthetic soaps to be much more effective in the washing. It would hold the stains and calcium and magnesium from re-depositing as fatty acids and phosphate salts on fabrics.

Anyway, they gave this to me because it was a chelator. Well, I started to look at it, and couldn't believe my eyes because it was a strong chelator. I just at first immediately started to look, "Well, what will it do to polished enamel?" I said, "It's going to destroy it because it's such an effective chelator—better than EDTA [ethylenediaminetetraacetic acid]." Well, lo and behold, it did not do any damage to the enamel. Then, I did grazing angle electron diffraction. I did electron microscopy. I did electron probe studies. Again, now I'd had all the background of the surface layer class with the fluorides. I said, "It's got to be forming some kind of an extremely thin film with the chelator that is totally protecting and blocking diffusion of the calcium and phosphate out and hence blocking damage of the enamel."

Then I started looking at it. Indeed it was a crystal-growth inhibitor. I did all of the crystal growth studies (12, 12a). "Well, if it's crystal-growth inhibitor, maybe it'll block calculus." All this work with calcium phosphate was in the back of my mind. Also, John Gray—I've got to give him credit for this, too. Both of us felt that we were learning so much about calcium phosphate and teeth and dentin, which is calcium phosphate also, hydroxylapatite—the difference being the amount of organic in enamel versus dentin—that the logical process would be bone next. I had my eye on bone in the future.

Well, I had devised an animal test for caries; that was the fluoride work (10). That took about a year or two, I guess. I toured around the United States at the top dental associations and got the top people—Dr. [Erling] Johanssen, Dr. [Raidar] Sognnaes, dean of the dental school at Harvard. We went to all of those places to learn about caries. Now, I'd devised an animal caries test in rats that was very short. We could produce white spots identical to those that were being produced in kids, in people. Then from there, with the twenty-fifth diet change, I devised a calculus model in rats so that they developed calculus, supra-gingival calculus, just like people did (13). Then, I had a model that I could go from the laboratory directly to the animal. Caries, I had that test, and then the calculus test. I found that I could inhibit calculus completely with this EHDP (14). Then in order to think about using that commercially, for people, we had to do all the safety tests. Well then, Bill [William R. King] and I did a really extensive biological study of the systemic effect of EHDP and found that it blocked bone growth. We published that in a kind of a landmark publication (15).

Then, we found out that it would block dissolution of bone. It was an anti-osteoporotic (16); it was effective in Paget's disease; it was effective in hypercalcemia of tumor origin; it was very effective in hip replacement, which can produce heterotopic ossification. That's how EHDP came into being. It was just a totally new concept of surface film protecting the bone. See, it wouldn't remove calculus. But if a person would go and have his teeth scaled and then used the dental product, he could block the formation of calculus. It was extremely effective if you didn't already have a huge accumulation. You couldn't remove it, because it was just as effective in blocking dissolution of calculus as it was in blocking dissolution of enamel.

TRAYNHAM: This compound was incorporated into the toothpaste, then.

FRANCIS: Yes. Well, no. Actually, I went from there to pyrophosphate and to polyphosphate. They were just as effective. The reason that they went that way was that by that time, we had a pretty good concept of the medical potential of EHDP, although there was nothing proved at that time. Then of course, we had this wonderful relationship. I went down to a dental meeting in Atlanta. I don't know, it was just sort of a habit. I would pick one of the top dental researchers in the field and invite him to breakfast or to lunch—somewhere early to get away quietly—then exchange information. I knew I had material that he wanted to know, and so on.

I took Dr. [James] Irving, who was at Harvard—he wasn't at Harvard at the time I met him. He was with Professor [Herbert] Fleisch in Berne at the Pathophysiological Institute. I had breakfast with Dr. Irving, and in the process told him about my work—I couldn't tell him about the phosphonates because that was company, privileged information, it was secure. I could tell him all about pyrophosphate and polyphosphate and how they inhibited crystal growth, and that they protected the surface, and that sort of thing.

In the process, then, he told me about his work with Fleisch in heterotopic ossification and calcification of the aorta with hypervitaminosis D. He and Fleisch were using hypervitaminosis D to produce aortic calcification, kidney calcification, to simulate stone formation, and a number of other things, and also, basically, atherosclerosis, in a way. The aorta would calcify. He and Fleisch were doing some work. Fleisch had a really terrific laboratory. Irving told me about his work with pyro- and polyphosphate in rats inhibiting heterotopic calcification. I told him about my physical chemical studies with pyrophosphate and polyphosphate—couldn't tell him about the phosphonates. He said, “Hey, you and Dr. Fleisch ought to get together.”

When I came back from the meeting, I talked to my section head [Dr. Griebstein] and said, “I'd like to invite Professor Fleisch into our laboratories.” So we did. In the process, my section head—I give him the credit for it—took the risk of telling Fleisch about our phosphonate work, which was very hush-hush. It was so exciting, because it had dental application, it had medical application—although P&G was not a medical company at that time. It was a detergent company. He took the risk of opening up. I sat with Fleisch and told him all about our work.

Then, we gave him four compounds. I wrote him an official letter and said, “This compound will work. This one won't. This one will. This one won't. This one will.” Then you see, he had these animal models—these systemic animal models—of calcification: aortic calcification, kidney calcification, heterotopic ossification, that sort of thing. He then gave the animals, systemically, these compounds. He wrote back and said, “I don't know how you did it, but every one you predicted would work, worked; and every one you predicted wouldn't work, didn't work.”

That set up a relationship between the University of Berne Pathophysiological Institute and Procter & Gamble. There, the key person was Mr. [Harry] Techlenberg. He was the director or vice president of P&G, I don't remember exactly what his position was. He became

senior vice president. He set up a program of large support, in the hundreds of thousands of dollars, with Professor Fleisch's laboratory. I shuttled back and forth and learned a lot of Professor Fleisch's techniques. Then, we set up our own animal studies in that area.

A Dr. [C.A.L.] Bassett from Columbia Presbyterian Hospital had a patient, a child, a sixteen-month-old baby girl, who had myositis ossificans progressiva. She was very much in danger of dying—not because of the pathological calcification per se, she was calcifying all of her muscles in the chest, but because of the calcification, it was totally inhibiting normal respiration. She was a diaphragm breather solely. She had caught a cold, and Bassett was afraid she was going to die. He called Fleisch and said, “Do you have anything? I've tried Imuran. I've tried this, I've tried that. Nothing helps this child.” Fleisch said, “Well, I can't tell you anything,” because he was under contract with us. He said, “But call Dr. Francis. Maybe he can help you, because he has some new materials.”

He called me. I invited him in immediately. I checked with my section head and said, “Can I invite him in?” I had him to my home, and he stayed with us overnight. He and I talked until I don't know what hour in the morning. Then, I took him out to the laboratory and showed him the physical chemical aspects. I had electron microscopy and a whole bunch of other things to show him. We went through it. I told him that I thought it had a chance of working on this girl. I wrote a letter, a memo. My section head wouldn't accept it. He said, “But it's okay. I'll send it on up.” I had said, “I recommend that we give this material to Dr. Bassett for this girl. It may save her life.” I went to the literature. It was a pathology—I'd never heard of it. It was really tough finding it. These people were hopeless—nobody knew what to do with them. They just were shunted off, you know. There was almost nothing in the literature. Well, I looked up what I could, did some mental gyrations, and figured that we could dose her, if it was okay. Sent it up the line. It went up P&G and was back down in thirty-six hours from Mr. [Gibb] Pleasants with the okay to treat the girl.

I stayed up almost three days running, trying to figure out a way that we could give it to Dr. Bassett for his patient. Couldn't give it to the girl with a powder. She couldn't take a powder. We didn't have any pills; they just were nonexistent. All we had was the raw material, so I formulated it in orange juice, and then proved that it still had its activity in orange juice. The work of Bill [William R.] King and me, and also Bill [William R.] Michael, had patterned where pathology started—in other words, excessive dose where pathology started (15). So we could move down into an area where it had the effect, but was not pathological in terms of blocking bone growth.

I did all the work on that. I went out to Columbia Presbyterian and gave him a vial of the material. Told him how to mix the orange juice, give it to the girl. Within two days she was in recession. He called me, told me what was going on. That was the very first human use of any diphosphonate.

TRAYNHAM: What was the time lapse between your getting the first information about the

girl in need of treatment and the administering of the treatment?

FRANCIS: Well see, he had to submit an application. My company said, “We can’t do it. We’re not a medical company. We can’t submit an IND [Investigative New Drug Application].” So we went to Dr. Bassett and said, “Would you submit the IND?” I gave him all the technical material he needed—all that I had at the time. He submitted an IND, and the Food and Drug Administration accepted it. There were about, I would guess, no more than two, two and a half weeks between when the vice president, who was Mr. Pleasants, made the decision. I don’t know. I still don’t understand why he made the decision. It was a terrible decision for him to make. Unbelievable. My memo was just a page and a half on the rationale. I gave him as much rationale as I had available.

TRAYNHAM: It must have been persuasive.

FRANCIS: He made the decision and said, “Go ahead, supply the material.” I did that formulation in orange juice and then took it out to Bassett. He treated the girl on their IND. As I say, within two days she was showing recession. She was very much in danger, but she lived. She was one of the few myositis ossificans progressiva who managed to live with the chest calcification. They all would die of respiratory problems, not of calcification. The disease itself would never have killed anybody; it was primarily one of respiration. Anyway, that was the first human use (17).

TRAYNHAM: Did she continue to get the treatment?

FRANCIS: Yes, intermittently. She would go into recession. He would take her off. Then when she exacerbated again, he would put her back on. I don’t know; I lost track after a while because he was getting the material directly. We then set up a medical section and got an M.D. I think our new first M.D. was Dr. [Blair] Geho. Then that’s really how she was treated.

One patient obviously means nothing—absolutely nothing—but then we went into heterotopic ossification with hip replacement. That was quite effective and is still used in some cases for hip replacement. A lot of hip replacements do calcify. It’s almost impossible to protect it when they have to cut that femur head off, and then put in the prosthesis. The little tiny, tiny particles of bone—they shield it as much as they can, but those little seeds can cause the muscles to calcify, then—and then they lose their range of motion and become immobilized. Then, it has to be replaced again. Now a lot of that has really improved in the last five, ten years from what it was way back then. We did heterotopic, and it was approved by the FDA for the use in heterotopic ossification.

Then we had a guy, Dr. [Andrew J.] Tofe, who was a nuclear chemist. He came to work for P&G. I don't remember exactly how he and I first got together. I was telling him about my stuff. Of course, he wasn't doing anything in that area. We were watching the literature, and Subramanian, I believe, was the first one who put technetium 99m, which is a 140 KeV gamma emitter, with a polyphosphate (18). Well, boom. The light went on. If it works for a polyphosphate—and polyphosphate wasn't very good, because it hydrolyzes so easily—diphosphonate should work.

Andy and I worked our fannies off. He was a great guy to work with—a tremendous, dynamic guy. He and I worked together and produced the first bone scanning agent with technetium-99m and EHDP, the diphosphonate (19). Then John Bevan came in. He pretty well perfected another diphosphonate that worked even better. That's the one that's still on the market now. That's methylene hydroxy diphosphonate or bisphosphonate as it is called now. That's combined with technetium-99m and tin. The Sn (II) reduces the pertechnetate (17) to a (+5) or (+4) oxidation state that chelates with the diphosphonate. Then it's taken to the bone in the circulating blood and deposits the technetium, so you can scan for any abnormality of bone metabolism. Things like arthritis, Paget's disease, heterotopic ossification, calcification of aorta—things of that nature will show up, because you can image the 140 KeV from the technetium wherever it's deposited. You can see—visualize—the whole skeleton (19).

Andy and I developed that. We had approval—at that time it wasn't the FDA, because it was a radiochemical. It must have been the AEC that had to approve it at that time. Anyway, within a year and a half, we had an approved product on the market. It's still being used. That was just plain fun. [laughter]

TRAYNHAM: I'm sure it was. That seems to be unusually quick for approval of a medical treatment.

FRANCIS: Oh, it was incredibly fast, yes. But you know, it was something they really desperately needed. They needed a stable product, and we had a stable product. We learned how to stabilize it by things like ascorbic acid, and published all of that work. Then, well, Bill Michael and I had published a paper on osteoporosis induced in rabbits, I think—goodness, I've forgotten—maybe rats (16). You cut the cruciate ligament so that the limb became immobilized. It was immobilization osteoporosis. We showed it very nicely, and published a paper that some of the diphosphonates would block that osteoporotic reaction.

Well, all of the sequelae then, EHDP is approved in twenty-two or twenty-three countries in the world—unfortunately, not in the States. The FDA would never approve it. But alendronate is approved, which is a diphosphonate.

TRAYNHAM: Why do you think the FDA has withheld approval of this?

FRANCIS: I don't want it put on there. [laughter]

TRAYNHAM: All right.

FRANCIS: I would say some very derogatory things, I'm afraid.

TRAYNHAM: Okay.

FRANCIS: It is approved in twenty-three countries: in Great Britain, France, Denmark, The Netherlands, Germany, Australia, New Zealand, Canada. All of those are approved, and a lot of others.

TRAYNHAM: Well, does that bring you up to date on your career?

FRANCIS: That's pretty much up to date, yes. Well, I'm working on a paper right now that I presented (15). I've had to publish it because I think it's a landmark paper. That's the thermodynamics that occurs underneath an osteoclast, which is the major cell that destroys bone. I haven't published it yet, but I presented it. I think it was a five-minute presentation in January of 1996 in Davos, Switzerland at the Davos meetings. What is it they're called? Not bone and mineral research. That's another. "Workshop on Bisphosphonates." Anyway, it's a meeting that's always held in Davos on bone. Then I presented it fully at Chantilly, Virginia, at an osteoporosis meeting there in September of 1996. I'm trying to get time to write it. That's up to date.

TRAYNHAM: Well, it certainly is a remarkable progression of your career: from teeth deeper and deeper into the interior, so to speak. [laughter]

FRANCIS: Well, you know, I have to credit John. He was very supportive. We kept saying, "We've got to get into bone; we've got to get into bone. We know too much."

TRAYNHAM: Was this your supervisor then?

FRANCIS: No, no. He was my colleague. John and I published together. I mean, we were always together. We shot flies off the wall together with elastics. [laughter]

TRAYNHAM: Well, you must have had very good relations with the research management to be able to be able to move from one topic to another so readily.

FRANCIS: Yes. I mean, P&G was not a pharmaceutical company. Any one of a number of times when papers would be given by outstanding clinicians—you know, on Paget's disease, heterotopic ossification, osteoporosis—they would say, "We're not a pharmaceutical company, but we ethically have to release this material because of what it's doing. Now, you know, millions—literally, millions—of people are using the diphosphonates for osteoporosis.

TRAYNHAM: Is that resulting in a significant budgetary impact on P&G?

FRANCIS: Well, it has been profitable, yes. Unfortunately, not nearly as profitable as could be, because we never got U.S. approval. They're developing a new one called Risedronate now, and of course Merck has come out with Alendronate. They are both new diphosphonates, which we've worked with, too. Alendronate is effective; there's no question about it. It's much more damaging to the GI tract. That's why I don't understand why the FDA—to me, it just doesn't make any sense. EHDP, or as we call it, Didronel—I mean, we've treated 2.1 million patient hours. We have one case, and that has to be a fluke. Alendronate has had a steady progression of severe GI problems. Basically, we knew that because it's a primary amino diphosphonate. For some reason or another, they're very hard on the GI tract—primarily esophageal inflammation and damage.

I think they're trying to get around it, and maybe are getting around it to some extent with a new regimen now. You know, you can't take it and then lie down, for instance. There are several other things that you have to be very careful in the regimen. Then, I think they have really reduced the number of problems that are involved with it. No question, it's effective. It's faster than Didronel in terms of its effect. To me that's kind of stupid, because osteoporosis is such a slow disease anyway. You don't need rapidity.

I think another problem is that they're giving it every day. Didronel is intermittent. Didronel is fourteen days on treatment, and then seventy-six days off. We call it ICT, intermittent cyclic therapy. It's fourteen on, seventy-six off, fourteen on, seventy-six off, et cetera.

Then, I did with another chap the calculations of what the body load of Didronel would be, proving that with that regimen, the body load would not proceed into the pathological area (20). All the diphosphonates will block bone formation if they're given at too high a level. Of

course, that's one of the other benefits of Alendronate that Merck has. It's much more effective: it has the same effect with a lower dose, is the better way to say it, I guess. With the lower dose, they don't move into the pathological area very easily. That is a definite positive part of the Alendronate picture, and very real.

[END OF TAPE, SIDE 3]

FRANCIS: A key factor in my career was when my major professor left, and left me stranded. I changed from an analytical major, with an organic and physical minor, to a major in biochemistry and a minor in physical. It just transformed my whole life, basically. At the time I thought it was a total disaster, but, you know, everything turned out very nicely. There are two major factors in my life: my wife's constant support, forty-seven years of support; she was an absolutely wonderful woman and gave me far more support, I think, than I deserved. The second major function—the second, maybe the third—was the constant influence of my father just very quietly emphasizing the need for education. I can remember him—just every once in a while he'd sit me down, and you know, not browbeat me at all, but just encourage me to go on. Then, my oldest brother has been my mentor. His incredible rise from ditch digger to a multiple manager of a huge pulp and paper organization. You know, scholastically, while he had a chance, he did just extremely well. As you well know, the first-born is always the smartest. [laughter] That's a given, almost. I'm the baby.

TRAYNHAM: I'm not sure the record in your family completely backs that up. You mentioned your brother was so successful in management. Did you ever have the opportunities to consider management positions?

FRANCIS: I was afraid you were going to ask me that. [laughter] Yes, they made me a group leader fairly early in my career. I hated it—I just hated it. I think I lasted a year. [laughter] I had one person reporting to me. I think he knew that I was out of place, and I certainly knew I was out of place as a manager. I have no desire to be a manager. None. As it worked out, it came out okay.

TRAYNHAM: Oh, yes?

FRANCIS: But my brother was just a real—I remember very little, funny little incidents. His strength was amazing. I don't mean physical strength—his mental stability. I mean, he basically supported my parents, while if you like, I was wasting my time in college.

TRAYNHAM: I'm sure you didn't hear that phrase from your father.

FRANCIS: Oh, no. No. Nor did I hear it from my brother. He basically supported my parents through some very, very tough times. For instance, I stayed with him in the bunkhouse. We were bunkhouse livers. I ate in the mess house with him. He paid for all of that, and supported me. I washed and ironed his shirts and pressed his pants, and washed the floor and made the beds. [laughter]

TRAYNHAM: Your early pre-P&G exposure to detergents.

FRANCIS: Well yes, I guess that would be pre-P&G detergent exposure, right. Anyway, he was just a very strong, very quiet man. I still just love him dearly. He's always been sort of the apex of where I felt I ought to go, if I could—just an inner strength. He's been married fifty-some-odd years, too. He's got a lovely—very, very nice, real feisty, real feisty—Scotswoman.

My brother Pat, again, he quit in grade ten, I think. Just couldn't see school—just couldn't see it. He wanted to get out into the world and make his way. He's been a stimulus too, because he did so well and was very successful. Not in the sense of creating products or shaking the world up, or politically contributing to Canada or the United States. Just a very, very successful businessman. Marvelous ability to deal with people. He's very well off, he has a lovely wife. So in a different way, he's been sort of a mentor, too. I have to admit, he's the one who really drove it home to me—to myself—that I did not want to have anything to do with salesmanship or commercial life. [laughter] Let me tell you, that pointed me in a direction so fast. I did well; I made money. I made more than enough money to go to school that next year, so it was profitable. But boy, I came to the conclusion that it was not my bag.

TRAYNHAM: Well, it was good to find that out early.

FRANCIS: I don't regret any of my logging camp—see, I worked in the logging camp on just about every aspect. I never did high rigging, but I did every other aspect from top to bottom. I started at the very bottom as a bull cook.

I don't regret my trip to the Queen Charlotte Islands. See, when I got my bachelor's, I didn't go to the logging camp. Right after my bachelor's, I got a job down at Canadian Fishing Company in the laboratory. They had a vitamin A analysis and a bunch of other analyses on fish products. I'd been working with them, I guess, about three, four weeks or something, maybe a little longer. I don't remember, because our summer was like from mid-May to September, when we were out of college. They had me pack up a laboratory and go up to the Queen Charlotte Islands on a houseboat, and set up a laboratory there for vitamin A analysis.

See, there was no synthetic vitamin A at that time. It was all from dogfish livers. They extracted it from the dogfish livers, and then they sold the vitamin A. Subsequently of course, now it's synthesized, but then it was all primarily from dogfish livers.

They showed me. I learned the analysis and all of the equipment that I needed. I packed up all the equipment, and went way up into the Queen Charlottes and lived on a houseboat—had to walk the boom sticks in to my “laboratory.” I had a little shack on shore where I did my extractions. I did the oil extraction. Then, I had a Klett-Somerson calorimeter which, of course, was passé many years ago, and did my analysis.

Their concept was, they were paying like a set fee, no matter what the fishermen brought in. The fishermen would bring in these big drums of vitamin A livers. They'd catch the dogfish; they'd gut out the livers and put them in the drums; and then they'd bring them in. They would pay, per pound, some set amount that would sort of fluctuate with the market. Well, their concept was, “Hey, it doesn't make any sense to do that. Why not pay them for the amount of vitamin A that they brought in.” The concept even now seems logical, but boy, did that stir up a hornet's nest. See, I was up isolated on this houseboat. I couldn't get off except to go into the little cabin, which was on shore, but nothing was there. I had a little cabin, and I had a stove on which I extracted the oil. I did my analyses.

Of course what happened is, the fishermen would hit a school. Well, that school might have a high content of vitamin A, or it might be a really low content. Well, you can imagine what happened when Fisherman A came in with five barrels, and got paid less than Fisherman B who came in with one barrel but had five times more vitamin A, or six times more.

TRAYNHAM: He would have done only one fifth of the work.

FRANCIS: But had done only one fifth of the work and had caught one fifth of the fish. It just—I really was a little worried for my safety. I mean, the company telephoned me, and they were very upset. It was a natural thing: they thought I was making very bad errors in my analysis, was the logical conclusion. You might say I was in the hot seat. [laughter]

They sent up a representative, and of course I had samples of all the things that I had analyzed. I put them in the refrigerator as soon as I finished. They took those all down, and they all analyzed out—obviously not the same numbers, but the numbers tallied. The low ones were low, the high ones were high, and the middle ones were in the middle. So they just shut the whole operation down. Then I went back down to the laboratory there. I guess I was up about a month and a half before you-know-what hit the fan. [laughter]

TRAYNHAM: Yes. When you completed your doctoral work, did you give any consideration to returning to Canada for employment?

FRANCIS: I did, but there was nothing in the area. It was a case where I could have gone East, but I was a Westerner by nature. So it was a case of, well, “Where can I do the kind of research that I want to do with the best possible chance?” Well, I could go East, but still in my opinion—

TRAYNHAM: It was a foreign country.

FRANCIS: Well, it was foreign country, but also, I didn’t feel the industry was up to what industry was in the United States. I did consider an academic position, but I wanted to do research. I desperately wanted to do it—I mean, I just loved it so much in my Ph.D. My professor was fantastic. He was really great. He taught me so many things—Dr. Winnick. I think he’s now dead.

TRAYNHAM: Did you become a U.S. citizen after joining P&G?

FRANCIS: Oh, yes. Listen, I was paying taxes. [laughter] At that time you had to have five years of residence in the U.S. At the end of the fifth year, bang, I became a U.S. citizen right then and there. My wife didn’t until about a year or two before she died. She finally became a citizen. She just never really wanted to give up her Canadian citizenship. We went back just about every other year. She went back to see her parents, and I went back to see my brothers. I still go back.

TRAYNHAM: Well now, based on your experiences, what does the term, “scientific innovation” mean to you?

FRANCIS: Well, I can tell you what it means to me. As you well know, there’s an old cliché—a well-known cliché—that innovation favors the prepared mind, so to speak. I think that’s maybe a parallel of it, anyway.

To me—I don’t think of myself as being innovative. I’ve been, I think, entirely stimulated by the question, “How the heck does this thing work? Why does it do what it does? What are the really basic elements of why something does what it does?” It almost amounts to following your nose. The more you ask a question and then answer that question, usually you’ve got five more questions at least, and those five have each five. Then, it’s never, “What to do.” I don’t think it’s ever, “What to do.” It’s, “What not to do,” that’s important. There’s never any limit to what you can do. I mean, you can work from now until the next century—the end of the next century—and there’ll still be things to do. But are they things that tell you how

something works, why it works, what's the mechanism? I love mechanisms. That's why I have been working on this paper now that I've already given. I want to get it into the literature, because it's so controversial that I know it's going to make a lot of people very unhappy.

TRAYNHAM: Well, it'll be stimulating to them.

FRANCIS: I think so. It's always been that I'm always reaching to understand what's going on. "What are the basic mechanisms? How does something work?" That's why I branched out so widely on the fluoride thing and how I was able to find the crystal-growth inhibitor. That was the key: when I found out I could crystal-growth inhibit calcium fluoride, which preceded the phosphonates. When the phosphonates came along, I recognized the fact just right away, you know, based on what I'd learned from the calcium fluoride crystal-growth inhibitors.

So I can't answer that question. I don't think of myself as innovative, but I do think of myself as wanting to know, "How does something work? Why does it work that way? What are the key fundamental chemical and physical principles behind how it works?" I think if you understand those, you're in a far better position to say, "Hey, that has application over here." If you're not thinking about mechanisms, you're never going to see the application that is totally different. I never would have thought of the application for medical when I was working on calcium fluoride. [laughter]

TRAYNHAM: In the course of your work in the industrial environment, were you mostly working as an individual researcher or mostly in a scientific team?

FRANCIS: You mean, which do I feel was more key?

TRAYNHAM: Well, what was your experience? Then, what are your thoughts about scientific teamwork in an industrial environment?

FRANCIS: Well, once I hit the dental group—up to that point I was pretty much an individual. When I did the skin work, I was sort of all alone in that area. When I did the hair work, there were people doing physical measurements—stress, strain, that sort of thing. I was pretty much alone there.

Once I hit that dental group with Dr. Gray, Dr. Griebstein, Dr. Grabenstetter, and Dr. Meckel, it was an unbelievably cohesive team of different disciplines. That was the key. I mean, we had a rate physical chemist. We had a dentist. We had an inorganic chemist. I was the biochemist. Bill Griebstein was a physical chemist; he was the section head of the group.

You couldn't tell one from the other. We were a team; there's no question about it. As I said, John Gray and I would sit shooting flies off the wall while we were discussing a fairly highly technical subject. [laughter] We were just very cohesive.

I made a habit at lunchtime—it's not true anymore, unfortunately—but I always sat with a different group. I would sit with the organic chemists. Then, I'd sit with the physical chemists. Many, many times, they would stimulate me to think about how something else might go. But our team was about the most cohesive team I think you could ever ask for in the world. We really made headway. I don't think there's a question in the world that we were the top dental researchers in the world, at the end. Not at the beginning. We didn't know anything when we began. We were a real bunch of klutzes. [laughter]

TRAYNHAM: Did you perceive any particular changes in the company R&D support or attitude about R&D during your career?

FRANCIS: No, not really. I don't understand why they supported me, because I was working in areas that I didn't see the commercial potential for. Maybe I'm kind of dumb, but I never really, totally conceived the commercial values of the things I was doing.

TRAYNHAM: You didn't have to sell that to the company management, then.

FRANCIS: Yes and no, you know. As it progressed, yes, but in the initial, I was just plain curious. I wanted to know how and why.

TRAYNHAM: Apparently, they had the wisdom to let you do so.

FRANCIS: Somehow they had the wisdom to let me go. I don't take the credit for that. [laughter] We had some very, very—I think—intuitive management. Bill Griebstein probably was absolutely a key in the management area. Mr. Techlenberg, he was an engineer. That's why he's not a doctor; he was an engineer. He was really the leading entity in terms of the research support. He was, I think, a director. Then he was a vice president, and then he was a senior vice president. I think he was probably pretty much alone in the management area.

I have to tell you a little anecdote that's kind of funny. John Gray, whom I really esteem—I really esteem him, and he was a terrific co-worker. But I can remember him saying on numerous occasions, "Dave, those phosphonates are the cancer of the company." [laughter] The company was spending money terribly on that, with nothing forthcoming at that time. John was very company-product oriented. He was a hell of a good physical chemist, but he was also

very practically oriented from the company standpoint. I can remember him several times telling me that I was wasting my time by working on those g-d diphosphonates, because they were the cancer of the company, and would always be the cancer of the company.

TRAYNHAM: You didn't perceive a threat to continuation of your research, though, did you?

FRANCIS: Well, I didn't pay any attention to him. [laughter] It wasn't that I really and truly was clairvoyant in any way, shape, or form. I wasn't. But they were such a fascinating group of chemicals—I mean, the way they worked and how they worked. Their mechanistic function in the medical area was just so interesting, I couldn't leave them. No way.

TRAYNHAM: I perceive from your remarks earlier in the interview that your research, and the team research, led directly to P&G's establishing a medical unit within the company.

FRANCIS: Oh, yes. That's what has established it.

TRAYNHAM: That continues today, then?

FRANCIS: As far as I know, yes. I hear there are terrible repercussions, with all of the downsizing. It's awful. I am really worried about P&G. I don't know all the details. I know that the stock is going up. But I'm afraid that P&G and an awful lot of companies are just buying their death—or at least their demise, serious demise.

TRAYNHAM: Do you think that's come about because of a different management attitude than what you perceived while you were there?

FRANCIS: Oh, yes, yes. I mean, I know so many people out there, from all my years out there. I was forty and one-half years with the company, and literally was in contact with just about every aspect of research that was going on at P&G. I would say, currently, there's almost no real basic research going on.

Part of the problem that I see differently, if you like—and I may be wrong, you know; don't mistake me. When it comes to making mistakes, I bet I've made more mistakes than most other people. But when I was there, development of products came from the bottom to the top. In other words, concepts were originated at the bottom. Now, I'm talking about Ph.D. level, technician level. They rose to the top like foam from a detergent. [laughter]

It seems as if all the research that's going on now is research that management says, "You will do this." I perceive that the discomfort with downsizing is so strong that even if people have an idea that this pursuit ought to be taken, if it deviates from the management perspective, they won't do it.

I got experience—a little bit of experience—in that area, the last three to four years. See, I was transferred to Norwich when I was in my sixties. I was at Norwich four years and had a wonderful period of time researching there. Then, I was transferred back to reestablish the bone group back here in Cincinnati. I was the first one back.

TRAYNHAM: Was your whole team transferred to Norwich?

FRANCIS: Yes. Everything was transferred—nearly everything. Not so much the developmental part, but all of the basic research was transferred. The entire bone group was transferred. When I got back—it didn't happen, bang, like that. But part of my joy was having ideas and working with brand-new people who had new knowledge that I didn't have. Like, I was working with an NMR person. NMR didn't exist for years while I was doing research. A lot of other people were coming in, new people with brand-new research tools and things like that. I would work with them and they would work with me happily, but if it were not an area that I was dynamically myself interested in, I would try to wean away from them. As soon as they were on their own and really making headway, I would try to wean myself away.

Time and again, I found as soon as I withdrew my support, they would quit. They would just drop the project. I don't really know why. The only conclusion I could draw was that they felt protected, because I had a pretty good name by that time. Like, we would write monthly reports or bi-weekly reports, which told the progress of how we were doing. As long as they could say, "Francis and I" or something like that, and they knew I would be writing about it from my standpoint, they felt fine. The minute I would try to withdraw and let them carry the project themselves, they were uncomfortable and felt threatened, because they were not doing something that was dead in line with management sort of—dictating may be too strong, but management concept of where research ought to be done.

I kept running into that. I went back to management, I bet, three different times. I said, "What the hell is going on with these young people, that they won't carry a project once it's started, and you know, not developed into anything, but looking to see and determine mechanisms of how things are operating? They'll quit as soon as I quit." If it wasn't directly where I wanted to work—because I had to carry my projects, too. As soon as I would—I guess you can only call it, "carry my support with them," they would drop it. That happened, I bet, four, five, six times.

TRAYNHAM: Did they find other mentors in the company?

FRANCIS: Not that I know of. They may have.

It was time that I would have to take out of my schedule. I'd have to allocate certain amounts of time to go over the data, help them interpret. I would help. I was interested in what they were doing, but it was maybe not directly in my line. I really worried about it. I spoke to management several times.

TRAYNHAM: Did the management have a response?

FRANCIS: Well, I never wrote a memo on it. I guess I should have. I didn't, because it was a perception. It wasn't even almost what you'd call a reality; it was a perception. I didn't mind passing my perception on to my section heads and my associate directors. I never went to the director—never jumped above them—because I never operated that way. I always, always tried to work through my section head.

[END OF TAPE, SIDE 4]

FRANCIS: The only time I went over it was, actually, the letter that I told you that I sent up. That was okay, because I'd given it to him. He said, "No, I can't support this, but I'll send it up if you tell me to." I said, "Yes, send it up." That's what started the phosphonate work.

TRAYNHAM: What, under these circumstances, do you consider is important for the future for chemical R&D?

FRANCIS: Well, I feel chemistry per se is really changing, you know. There is so much instrumentation today that is literally removing the chemist—as I knew a chemist—from the field. I'm not talking chemical industry now, but rather chemistry per se, I think, is in the decline. Not instrumentation, not electronics, not all of the new things that are coming up that are sort of removing the chemist from the field as a hands-on chemist—I guess that's the best way of saying it. I think it's changing radically.

I don't know whether the question you're asking is, "What do I think is wrong?" I really feel that innovation has to come from the line: from the people who are at the source of how things work, why they work, and how they operate. It has to go from there up. I have no problem with management saying, "This is a broad area that the company's interested in. You

find a project in that area.” I have no problem with that. But for the management to pretty much pinpoint what the Ph.D.s are going to do in the laboratory—almost dictating, not quite, but almost—it’s just the wrong direction. Management is not in a position—they’re just not in a position—to know, to strongly dictate the direction of research. I think that has to come from the line, if you understand what I mean by the line.

TRAYNHAM: Yes, yes.

FRANCIS: The people at the source of the development of the knowledge are the line for me. That’s usually the Ph.D.s, the masters maybe, in there. That’s the way I feel. I feel strongly that there is too much from the top down, and not nearly enough from the bottom up.

TRAYNHAM: You perceive that to be a very significant change from your career days, then.

FRANCIS: That’s a real difference now. I tried in my Perkin Medal talk to emphasize the factor of fear. I had no fear when I was working. But let me tell you, fear today is rampant among the Ph.D.s. I don’t mean they’re scared of living or of somebody shooting them, or anything. I’m talking about a fear that if they don’t conform to a certain research pattern, they’re out. So they won’t take risks. I mean, why the hell do you work on something that you almost know the answer? There’s no fun in that. Why do it? It’s already—you almost know the answer. You’ve got to go into areas that nobody—that’s true of me. I didn’t know where the phosphonates were going to be. Now, don’t make that mistake. I didn’t see that product. I didn’t see millions of dollars in osteoporosis when I first started. Heck, no. I’m not that smart. [laughter] But it was tremendously scientifically interesting. That’s what I’m talking about. I see that missing today.

TRAYNHAM: To phrase this inquiry a slightly different way, the editor of the *New York Times Book Review* at year end, each year, publishes a list of a hundred books judged to be the most significant among those published in that year (21). Last month, included in that list was one by John Horgan entitled, *The End of Science* (22). A capsule summary of the book read: “A bracing argument by a science journalist that the best and biggest discoveries are behind us.” What’s your reaction to that?

FRANCIS: No, I don’t think so. I think if you’d gone back to my time, I could have said the same thing. I don’t think so, mainly because the volume of knowledge, I don’t think is decreasing. I think it is still increasing. If we’re at the place where we know everything, yes, then science will be finished. I don’t think we’re there yet. I think we’re a long way from there. I certainly don’t know everything that’s going on in my little field.

It's different. You know, the mechanisms of finding things are different. Instrumentation now is so much more advanced than it was in my day, although, boy, I sure used instrumentation to the fullest—the latest and best methods. But I don't think so. I don't think we're near saying, "We know everything that there is to be known." Until we reach that point, I don't think there is much difference. I wouldn't mind starting research all over again. [laughter] In fact, I wish I could go back, but age seems to have a way of creeping up on you.

TRAYNHAM: Yes. Well, during your career you received several significant awards.

FRANCIS: Oh, yes.

TRAYNHAM: Could you comment on those? What impact did they have on you or your work, or on your recognition within the company?

FRANCIS: Well, I was always kind of dumbfounded when they gave them to me. [laughter] The very first one was the ACS award that I got.

TRAYNHAM: In 1994?

FRANCIS: Oh, no.

TRAYNHAM: In 1977?

FRANCIS: In 1977, I guess.

TRAYNHAM: That was the Cincinnati Chemist of the Year Award, I believe.

FRANCIS: Yes. That was the very first one, I think, that I had. It was very nice, you know. I just viewed it as, as I said, being very nice. I was not quite sure why I got it. The second award was the Technical Scientific Societies Council of Cincinnati that gave me the Professional Accomplishment Award in Industry. Again, that was a total and complete surprise.

You know, if you come right down to it, one of the reasons that I've gotten any of these

things is that I've had people who put me up for it. I wouldn't have ever put myself up for them.

TRAYNHAM: No.

FRANCIS: Dr. Ted [J.] Logan, for instance, has, I'm sure. Well, the people in the company, obviously, were responsible for my Victor Mills Society Award in 1990, which is the highest P&G award that can be received for innovation. The inorganic ACS award that I got—you know, that's the national award that I got in San Diego in 1994—and the Perkin Medal, I don't think they really belong to me. They more belong to Ted Logan and the people— Dr. Anne Geddes—who put all of it in. I didn't even know they were doing it.

Awards, they're very nice. They're wonderful. Don't mistake me; I don't downgrade them at all. But they really belong to the people who put a tremendous amount of effort into documenting, so that you can even be considered for the award. I don't know how you handle that sort of thing. Ted's a fabulously nice guy—and his secretary, who did a lot of his elbow work. She's wonderful, Marlene Roper. They're the ones. Dr. Ray D'Alonzo was obviously responsible, too, for a lot of that.

The Morley Award and Medal that I got after the Perkin—see, I got the Morley Award from Cleveland. That must have been largely due to Ray D'Alonzo. I don't know—I mean, they're nice. Don't mistake me. I appreciate them; I really do appreciate them. But in some ways they belong to the people who did all the work. Believe me, that's not inconsequential.

TRAYNHAM: No, it's not. A lot of credit for awards certainly belongs to the effort of those who put it together.

FRANCIS: Absolutely, yes.

TRAYNHAM: The award itself, which recognizes the research you did, belongs to you. However, I agree that without friends to put it together as a nomination, you'd be left behind.

FRANCIS: You're dead. You're dead in the water. [laughter]

TRAYNHAM: Your Perkin Medal came after your retirement, so it's hard to perceive if it had an impact on your career. I'm sure it was wonderful to receive, though.

FRANCIS: Oh, it was wonderful. Well, it was especially wonderful because my daughter went, and my son went. Actually, Candy [Candice] Slough also went. Candy was one of my top technicians.

That's another thing that's changing today that I don't understand. I wouldn't have any of those awards if it hadn't been for my technicians. Let's face it, they did most of the work. I mean, I used my head a little bit. I had hands-on work too, a lot of times, handling really miserable, ugly rats—vicious rats. But the nitty-gritty of the real valid work—the substantive research work, the hands-on work—in my case, has been done by just fabulous technicians. I've always tried to recognize these people and the role that they play. Like Bobby [Robert] Barnett, whom I considered a very bright guy, has no technicians. How can you produce volumes of work that will ultimately yield something concrete without plenty of help?

Now you know, at times I've had three technicians. Now, very few other people have ever had three. They were lucky if they got one or two. I can't underplay the role that technicians have played in my career. I can't underplay that. They've been tremendously valuable. Several times when I've been giving a talk at the company, I have tried to say those kinds of things, but I don't think they hear you. If they're bright technicians—and I've always been blessed by incredibly good technicians. Mind you, I've spent hours and hours and hours training them, and training them in a way where they knew why they were doing something. They weren't just doing things; they knew why. I made sure that my technicians knew as much as I did. In some cases, like with Ron [Ronald] Montgomery, he knew more than I did. I mean, he developed to the point where he surpassed me in a certain area. There's no question that he did. In those days it was very hard: the jump from technician to staff, if you didn't have a Ph.D., was very tough. He made staff on his own and stayed staff until he retired.

I don't take credit for it all, at all. Without those really top technical assistants—I don't know. I was verbally and loudly against this new system that they've got of raising technician levels one, two, three, four, five, six, seven, eight, I guess. Eight is the top. That's the one that I achieved. But you know, there was an aura all through the time that I worked that technicians were inferior. They're not—never have been. They were inferior if the staff person in charge of them didn't teach them—didn't tell them why—but said, "Do this, do that," but never shared the interpretation of the data, the write-up of the data, and so on. See, I co-published with a lot of my technicians because they contributed. I see that as a change.

TRAYNHAM: That's very generous credit you give to technicians.

FRANCIS: No, it isn't. No, it isn't generous.

TRAYNHAM: I'm sure it's heartfelt.

FRANCIS: It isn't, because I know I couldn't have done what I have done. I couldn't have. It's absolutely impossible without the top people. I mean, Candy Slough and some of the other technicians that I had just did absolutely wonderful work. You know, it was always my habit to do something one way and then do it another, which would give me a clue that they were giving me valid, solid data. There was only one technician whom I found a flaw in. I got rid of her in a hurry. But you know, that was a given. You did things one way. Then, you turned around and did them in another way that would verify what you did initially. Actually, in my early career I was criticized rather severely for that—I tried to button things up too tightly. I just ignored it and continued to do what I had to do. [laughter]

TRAYNHAM: You have made reference a time or two to your family members. Is there anything else you would like to tell us about your family?

FRANCIS: Well, just that I've had a marvelous family, you know. You know, when my wife died it was a very tough period for me. My two kids just backed me up. It maybe had nothing to do with my career, since she died in 1995 and I retired in 1993. But I've always admired my two kids. Bill [William R. Francis], my son, is very bright—really bright. I'd like to have the brains that he's got. He ranked in the 97th percentile in the nation in his SATs when he wrote them. My daughter—well, she hated school. [laughter] My daughter just hated school, mainly because she was in a bad situation. It was a school that was bad. You know, what you see between my fingers is how much she cleared her graduation from high school. She couldn't wait to get out. I sent her through the Potomac Riding Center in Washington. Half of her at least is horse, and the other half is human. [laughter] I bought her a horse when she was—I forget what birthday it was—twelve or thirteen, something like that.

Anyway, I sent her to the Potomac. She graduated with high honors from the Potomac Riding Center, so she had her horsemaster's degree. She then went as a groom back East, you know, which is real horsey country. Then, she became groom for Jackie [Jacqueline Kennedy] Onassis. She taught John-John [F. Kennedy, Jr.] and Caroline [Kennedy] how to ride. She was with the Onassis. She was an Onassis at that time; she wasn't a Kennedy. Anyway, when she came back from there, she went back to school. The first semester, she was on the dean's list. The second semester she was on the president's list, which was straight A—from having, as I said, "What you see between my fingers is how much she cleared in her high school."

It's the old business of, if you're unhappy, you don't do well. That's what I fear in what I see of today. Now, you know, I don't see everything, but what I see at P&G, and what I hear from other people whom I know technically from other companies is, people are really unhappy—really unhappy. They can't wait to retire. I couldn't even consider retiring—didn't want to consider it. My wife was the one who finally got me to. She beat on me for about five years. [laughter] Unfortunately, she died shortly after.

TRAYNHAM: It was her requirement rather than the company requirement that you retire when you did?

FRANCIS: As far as I know, yes. I mean, nobody ever asked me to quit. I have a tape downstairs of my retirement where one of the associate directors, who is sort of an emcee, at one point in my retirement thing stood up and said, "I don't understand how Dr. Francis can have a retirement party when he's not retiring from the company." [laughter] That was when I went from Miami Valley Labs in Cincinnati in 1985 to Norwich, New York. They gave me a retirement party. When I left Norwich, they gave me a retirement party. When I came back to Cincinnati and quit, they gave me a retirement party.

TRAYNHAM: You retired three times.

FRANCIS: Three times.

TRAYNHAM: You still want to continue research, then.

FRANCIS: I would have loved to have continued. I know in my inside that I was on the track of a cure for osteoarthritis and rheumatoid arthritis. I know I was. I don't know, I might actually be wrong. I've been wrong many times before. [laughter]

TRAYNHAM: Well, that's a good track to be on.

FRANCIS: I know I was on the right track. Unfortunately, it has languished. When I finally quit, I was right at what I thought was the apex. I could feel it. You feel these things; you don't know them, you feel them. I know I was there. Another two years, and I think I would have had a cure for at least rheumatoid, and I think even osteo. My data all said yes. That was animal data, too. Maybe it will be picked up eventually; I don't know.

TRAYNHAM: Your daughter has returned to college. What is she doing now?

FRANCIS: Well, she went into respiratory therapy. That's what she got her degree in, and practiced, I guess, respiratory therapy in New York. She and her husband moved to New York for seven years. He was pursuing his Ph.D. She worked at hospitals in New York. Then they

went to Texas, and she worked in hospitals there.

I guess she changed her life work partly because of me. She was apparently extremely effective with AIDS patients. Her AIDS patient load went up and up and up. I remember when I was down in California, she was at the hospital—they moved from Texas to California, afterwards—I said, “Pat, I know that these people have to have somebody to take care of them. But if there’s any way you can get out of it, now is the time. You’ve spent almost twenty years with it. It’s just a matter of time before they’re going to cut their finger or something.” She would describe some of the things that happened with these AIDS patients with respiratory—not the AIDS per se, but the respiratory problems—infections and so on. God, it just scared the daylight out of me as a father. When her husband upped and walked out on her—she’s here in town now and pursuing a different field. She’s in sales in the health area; I don’t know, exactly. I don’t know much about finance, but I guess you’d call it finance: selling packages of retirement plans for maximum benefit at retirement. It’s a tough area, that is. Boy, is that a tough area.

TRAYNHAM: I believe you said your son is employed at P&G?

FRANCIS: He’s at P&G. He’s at P&G in chemistry.

TRAYNHAM: Is he in research there?

FRANCIS: Yes, he’s in research. He’s currently working on—well, I think you’ve heard of Olestra. Does that ring a bell?

TRAYNHAM: Yes.

FRANCIS: Olestra is thought to remove some of the fat-soluble vitamins. He’s working on what do they do about this. Do they reinforce it so that a certain percentage stays in? He’s working out a lot of the kinetics of these. I think the main one now is beta carotene that they’re worried about: vitamin A, vitamin B, and those things. That’s what he’s doing currently. It’s mostly animal work, much like I did.

TRAYNHAM: What are you spending your retirement doing?

FRANCIS: I love to teach. I really do, but all my forty and one-half years—see, I taught

school. Like, when I was at the University of British Columbia, I guess you'd call me an assistant to the professor. I did a lot of teaching there: laboratory work and lecturing and so on. I really love teaching. I guess maybe I'm a little scared that teaching techniques have changed so radically. So much visual material is used now as opposed to standing up to the board and writing things on the board, or handing out copies of things. Then my wife's death shook me up. I was close to trying to get a position with the Xavier University or the University of Cincinnati when her death came along. That kind of shook me up a little bit.

I haven't really pursued anything. I'm consulting a little bit, but I don't actively seek it. I find consulting very unsatisfying—extremely unsatisfying. When consultants come in, they can suggest A and B and C. When they leave, A and B and C could be totally ignored. They come back the next time, and the first thing they say is, "Well, what about idea A and B and C?" "Oh, well, we decided to go bypass that." Well, there's no gratification in that. If you don't do A and B and C, you can't find E and F and G. [laughter]

TRAYNHAM: Well, you're a golfer, I believe.

FRANCIS: Well, I walk around the course. My golf game's different now than it was. I got bitten by a tick, and *evlichia chaffiensis* got me. I went into respiratory failure, and I guess I passed the recovery point. They thumped me with paddles about five times. They finally got me back. I guess partly because I was in such good condition, I did survive. But now if I hit a ball into the woods, which is not that infrequent, I just wave it goodbye and drop it, and hit another one. You might say my golf game is pretty passé.

I'm more of a tennis player. I really like tennis. I like the tactics, and I love the companionship. In fact, that's one of the things I'm doing in February. My tennis group, the four of us, four-man, we play. I have a partner, and he and I are actually in tournaments together. This foursome is going down to the Bahamas. We're going to have three days of solid tennis while we're down there, plus loafing around the beach. I'm very active in sports. I've got a ping-pong table downstairs, and I love to play ping-pong.

TRAYNHAM: Well, thank you for being so generous with your time, and for telling such an interesting story about your career.

FRANCIS: I'm not so sure it's interesting, but I've no regrets, none whatsoever. With all jobs, there is what I call scut work, but a major portion of my research career has really been fun. There were times when I felt guilty, I was having so much fun. [laughter] I mean, really and truly. I'd come home and tell my wife, "I can hardly wait to get back to the lab tomorrow, because I know I'll get the results from such-and-such." It was just plain fun. Plus, the real benefit is the people I worked with who were so helpful—jeepers, my technicians, my

associates.

The composite of my work is a composite of the people I worked with—no question about it—and the people who supported me, and my wife, and my kids, and my grandchildren. [laughter] I've got four grandchildren. One of them is twenty-five and married. That's my son's eldest—elder, I should say. Then he has a thirteen-year-old girl.

[END OF TAPE, SIDE 5]

FRANCIS: Actually, she is now eleven, I think, or ten—ten or eleven—close to eleven. She'll have a birthday pretty soon. My family life has been very happy. I had a wonderful family life growing up. I had great brothers. Just a lucky son of a gun, that's all. Like I say on the tennis court when I make a really good shot, "I'd rather be lucky than good any day." [laughter]

TRAYNHAM: Is there anything else to complete the picture here for the recording?

FRANCIS: Oh, lots of other little vignettes. But, as I say, the key people in my life were my wife, my children, people whom I worked with, my brother, who is a real—I think it was my brother's approach to life that helped me so much. He was so stoic—maybe that's not the right word. Solid, I guess, would be the right word—really solid. My father and my mother. I can't negate her at all. She was tremendously active right up until the time of death. I'll show you a picture of her ninety-seventh birthday. Right there. My oldest brother, my middle brother, myself. This is his wife; that's my wife; this is his wife. This is my cousin, and there's my mother. Wonderful woman. Mother was fantastic. That's it.

TRAYNHAM: Thank you.

[END OF TAPE, SIDE 6]

[END OF INTERVIEW]

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